The *Ornithorhynchus* being an aquatic animal does not possess a pouch at any time. With respect to the New Guinea species of *Echidna*, the question whether the *Tachyglossus lawsei* and *T. bruijnii* are distinct species can now be decided, as I observe that examples of both sexes of *T. bruijnii* have been obtained in the mountains on the north coast of New Guinea at an elevation of about 3,500 feet. That a new and somewhat analogous species of *Tachyglossus* may yet be discovered in Northern Australia I consider very probable. GEORGE BENNETT

September 29

P.S.—By letters from Sydney dated August 4 no intelligence has been received from Sig. D'Albertis since his departure for the Fly River in May last.

Are there no Boulders in Orkney and Shetland?

IN your impression of the 13th inst. (p. 418), there is an interesting letter from Mr. Laing, M.P., stating that there are no boulders in Orkney or Shetland. He says that having "an intimate personal acquaintance with these islands, which are my native county, and almost every yard of whose surface and shores I have explored with rod and gun," . . . "I can assert positively that I never saw (in them) a boulder or perched block, or the trace of any till, or boulder clay, kame, eskar, raised beach, or other form of glacial or marine action."

Mr. Laing's object in drawing the attention of geologists to these facts is, that "if true, they seem to afford a crucial test of the truth or falsehood of some of the most important theories of modern geology."

Mr. Laing observes that in Orkney there could be no boulders, &c., because "there were no glaciers, there being no great local mass of mountains to produce them."

As regards Shetland, Mr. Laing says he cannot speak with the same confidence. "Still, having travelled over a great part of the principal islands, I can assert that I have never seen in them either, any traces of glacial action."

Mr. Laing having invited information on this subject, Prof. Geikie has published an article in the same number of your paper (p. 414), controverting Mr. Laing's statements, and maintaining that the facts ascertained by him and his colleagues in the Scotch Geological Survey establish that these islands form "no exception to the general glaciated condition of Scotland."

In corroboration, so far, of the Professor's statement, that there are in Orkney and Shetland "many transported blocks of gneiss, schist, and other rocks foreign to the immediate locality" of the blocks, I need only refer to the following list of boulders reported to the Edinburgh Royal Society Boulder Committee.

IN ORKNEY.

Eday Island.—Conglomerate B. 12 × 7 × 1½ feet, = about 8 tons weight. Situated near top of a hill 250 feet above sea. No conglomerate rock in Eday, but there is in Stronsay Island.

Frith and Stennis.—White pebbles of freestone on the hills. But there are no white freestone rocks in this island. It is all old red.

Sanday.—Gneiss B. 7 × 6 × 2½ feet = about 14 tons. No gneiss rocks on this island. Nearest island with gneiss rocks is Stromness, 30 miles distant, and in Shetland, still more distant.

Walls.—Lydian stone B. 9 × 7 × 6 feet = about twenty-eight tons. Sandstone is the prevailing rock.

Stromness Island.—Two granite boulders lying on red sandstone rocks—distant, one a quarter of a mile, the other one mile from granite hills.

IN SHETLAND.

Bressay.—A number of boulders here, of a rock foreign to the island. One of them is 10 × 7 × 4 feet. Supposed to have been transported from Norway.

Housay.—On a cliff 200 feet above the sea, rounded blocks resting on knolls of polished rock.

Neay.—Large perched blocks, some many tons in weight.

Sumburgh Head.—Conglomerate B. lying on sandstone rock.

Where can it be supposed that these boulders come from?

Prof. Geikie thinks there were glaciers, at all events, in one of the islands, viz., *Hoy*, and even "separate glaciers" in all the valleys of that hill, whose top is only 1,550 feet above the sea. I feel great difficulty in subscribing to that opinion; I rather agree with Mr. Laing, that there could be no glaciers, for want of a sufficient "mass of mountain region to produce them." Even if in Hoy glaciers could have been formed on a hill the highest peak of which is only 1,550 feet above the adjoining sea, what is to be said of those boulders which are on islands where the hills do not exceed 500 or 200 feet, and in which there are no rocks of the same nature as the boulders?

Prof. Geikie refers to the rock striations in Orkney and Shetland (which Mr. Laing seems not to have discovered) as additional proofs of glacial action. If these striations had been caused by glaciers, the direction of the striæ would vary with the direction of the different valleys in which the glaciers moved. But this is found not to be the case. Prof. Geikie says that both in Orkney and in Shetland the movement of the ice has been on the "whole along a north-west and south-east line." He refers to reports by his colleagues, Mr. Peach and Mr. Horne, in corroboration of his statements.

In looking into Mr. Peach's report, I find that he specifies the striæ on the rocks of Shetland as running in *various* directions. In *Unst*, the most northern island, he says "the destroying force (the nature of which force, however, he avoids indicating) coming against a hill (called the '*Muckle Heog*,' 500 feet high) on its north-west flank, had been partially *turned* by the hill into a valley (which he names) and made to produce the well-known phenomenon of 'crag and tail'"—the crag or bared rock being on the north side of the hill.

Mr. Horne in his paper also describes the striæ in Shetland as running in *various different* directions. Some of the striæ on the rocks, and the boulders on the surface, indicated, as he thought, ice action from east to west. "In addition to these, however, *others* (he says) were found, which could not have been produced by ice-shedding in the ordinary way. These cross the island, *regardless of its physical features*, and are *often at right angles* to the newer set."

These facts, I venture to submit, may be explained by supposing that the Shetland and Orkney Islands, when still under the ocean, were subjected to the action of Arctic currents loaded with icebergs and shore ice. We know that in the Arctic regions now, fragments of rocks are by these means carried about in various directions, and dropped on the sea-bottom; whilst the rocks at the sea bottom are ground down, polished and striated by the hard stones and gravel pushed forward by icebergs. The existence of Arctic currents from north-western points has indeed been already well established by a study of the boulders and striated rocks found along the west coasts of Caithness, Ross, Argyll, and the islands of Lewis, Harris, and Uist.

The inference of Mr. Peach from what he saw near Lerwick was, that there "the *direction* the *drift* came from is evidently *northerly*." "The destroying force" to which Mr. Peach refers as having swept across the island of Unst baring a hill up to a height of 500 feet *on its north-west* flank, could have been no other than an Arctic current loaded with ice.

These facts establish points of the highest geological interest. They indicate a submersion of the northern parts of Europe under the ocean to the extent of many hundred feet, and the non-existence of any gulf-stream flowing through the North Atlantic. The Isthmus of Panama requires to be depressed only 350 feet, to allow that stream to flow into the Pacific.

The separate question of "*Raised beaches*" mooted by Mr. Laing and discussed by Prof. Geikie, I do not enter on. Both of these authorities agree that there are no raised beaches in Shetland and Orkney. It is indeed very curious that such should be the case, considering that they exist along the Caithness coast, and in every other part of the kingdom, including Ireland. I may, however, notice that Mr. Peach in his Report on Shetland speaks of a "raised beach" as having been seen by him there.

Milne Graden, Coldstream, N.B. DAVID MILNE HOME

Fertilisation of Flowers by Birds

AMONG the "Biological Notes" in NATURE, vol. xv. p. 416, there is one referring to the agency of birds in effecting the fertilisation of flowers. A few weeks before reading this note I was induced to suspect that many flowers might be dependent wholly or in part on the visits of small birds for their effectual fertilisation by observing that a very considerable number of birds shot at that time had the plumes of the forehead and the lores thickly dusted with pollen. This fact was noticed in several species of *Dicæinæ* and *Nectariniinæ*, in the *Loriculi*, and even in a glossy starling (*Calornis panayensis*), which latter is mainly a frugivorous bird. Both the sun-birds and flower-peckers are fruit-feeders to a certain extent; but they also prey on minute insects, in search of which (and possibly of the nectar sometimes) they diligently probe the corollas of numerous flowers, and on withdrawing their heads a portion of the pollen remains in many instances adhering to the plumage bordering the bill, and is carried away and introduced into the next blossom visited.

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The immediate source of attraction possessed by the flower for its feathered visitants lies, I think, in the small insects which resort to it, and not, at any rate usually, in their furnishing any nectareous secretion which is palatable to the birds. For if the latter visited the blossoms for the sake of the nectar they would be perfectly acquainted by experience with its situation and make no delay in going straight to it, whereas the habit of the sun-birds and the flower-peckers also is rather to hover on rapidly-vibrating wings a few inches in front of the opening of a blossom, as if prying into its recesses in search of food, before thrusting their beaks into the corolla; and often after thus examining a flower they fly off to another without touching it at all, having apparently satisfied themselves that the first one contained no prey for them. A. H. EVERETT

N. Mindanao, July 23

Heat Phenomena and Muscular Action

ON reading the article which appeared in NATURE, vol. xvi. p. 451, on the heat phenomena accompanying muscular action, it has occurred to me to send the following problem which is akin to the subject.

If a man does work (say lifts a weight), the principle of the conservation of energy teaches us that the potential energy—the work done—(weight lifted) is at the expense of the man as a magazine of force, in fact that "virtue has gone out of him." Now suppose a man lifts say a ton of bricks and deposits the bricks one by one on the top of a wall six feet high, we can exactly estimate the amount of work done, the energy rendered potential and external, and if we knew also the extra amount of heat radiated or otherwise carried off from his body—as most probably the work would raise his temperature—we could exactly measure the amount of energy the lifting of the brick cost him.

Now suppose another man were to lift the bricks from the top of the wall and deposit them gently—*i.e.*, without concussion—on the ground, it is evident that there is a certain amount of potential energy disappearing, in fact that there is work being absorbed by the man, of course appearing in some other form, but the question is how? This second man's work is of course in one sense work, but in the sense of producing external, potential, or kinetic energy, is not so, unless, perhaps, in heat.

Strangely enough it follows that lifting down the brick ought to make the man either radiate heat more, waste tissue less, digest food less, or in some other way account for the energy absorbed by him.

Generally I think the conversion of force by obstruction is not always so clearly traced as it might be; in friction it is clear, as also in the compression of elastic bodies, but in the instance above, as also in the throttling of steam, it is not so clear. A. R. MOLISON

Does Sunshine Extinguish Fire?

I READ Mr. Tomlinson's paper (NATURE, vol. xvi. p. 361) near the time of its delivery, and was struck with the inconclusive character of his experiments. What he attempted to obtain was the condition of combustion in sunshine and combustion in darkness, *cæteris paribus*. But he left the *cæteris paribus* entirely out of the experiment, and actually used a dark cubbard (I believe this is good spelling etymologically and phonetically), into which there was no free influx of atmospheric air. Naturally his candles burnt with inferior combustion there. I have for years together burnt Newcastle coal, and no other; and for years together burnt South Staffordshire coal, and no other; and I say that sunshine puts out a sea-coal fire and not a S.S. fire. The reason of this is, I apprehend, not far to seek. In the Midlands it is the practice to keep a fire alive by a raker, or gathercoal. It would be quite useless to attempt to do this with a sea-coal fire, which goes out in a short time unless the cakes of coal be broken up; in a word, one has to watch a sea-coal fire; and it must be in every Londoner's experience, that such a fire is apt to elude one at the last faint gleam from over reckless poking. Now, if the sun is shining on the coal, that last faint gleam is invisible, and the fire goes out as a matter of course. Sunshine puts out a sea-coal fire by insidiously eclipsing the warning glimmer of its expiring embers. This, at least, is a *vera causa*. *A priori* I should say that combustion would be less rapid in air rarefied by sunlight than in air deprived of it; but I do not believe sunshine extinguishes a coal fire in any other way than that I have described. C. M. INGLEBY

Folkestone

OUR ASTRONOMICAL COLUMN

THE APPROACHING OPPOSITION OF IRIS.—The opposition of this minor planet in the present autumn affords another favourable opportunity of determining the amount of solar parallax on the method already successfully applied by Prof. Galle, of Breslau, in the case of Flora. The *Berliner astronomisches Jahrbuch* for 1879 contains a rough ephemeris of Iris for every twentieth day of the year, but this being insufficient for the purpose in view, we subjoin places calculated from Prof. Brünnow's tables of the planet, on the approximate formulæ explained in his introduction; the error of the tables being very sensible at the present time, nothing would have been gained by calculating in the accurate form. For the sake of brevity the planet's positions are given for every fourth day only, but they will be readily interpolated for the intermediate dates.

IRIS.—*At Greenwich Midnight.*

	Right Ascension. h. m. s.	North Declination. ° ′	Distance from the Earth.	Distance from the Sun.
Oct. 8	3 56 7	27 5·6	1·0034	1·8350
,, 12	3 56 50	27 3·3	0·9769	
,, 16	3 56 55	26 57·8	0·9526	1·8345
,, 20	3 56 20	26 48·9	0·9306	
,, 24	3 55 8	26 36·5	0·9111	1·8354
,, 28	3 53 20	26 20·5	0·8944	
Nov. 1	3 50 58	26 0·9	0·8808	1·8376
,, 5	3 48 8	25 37·8	0·8704	
,, 9	3 44 57	25 11·6	0·8633	1·8411
,, 13	3 41 32	24 42·7	0·8597	
,, 17	3 38 1	24 11·5	0·8597	1·8459
,, 21	3 34 31	23 38·7	0·8634	
,, 25	3 31 9	23 5·0	0·8708	1·8520
,, 29	3 28 2	22 31·2	0·8819	
Dec. 3	3 25 17	21 58·1	0·8965	1·8593
,, 7	3 22 58	21 26·5	0·9146	
,, 11	3 21 11	20 56·7	0·9360	1·8677

Iris will be in perihelion October 14·7, G.M.T., and nearest to the earth on November 15, her distance at this time being 0·859 (the earth's mean distance from the sun being taken as unity). Her intensity of light may be expected to rather exceed that of a star of the seventh magnitude, 6·8m. according to the *Berliner Jahrbuch*.

THE OUTER SATELLITE OF MARS.—This object is still under observation at the Observatory of Paris. It was also measured again by Mr. Common, of Ealing, with his 18-inch silver-on-glass reflector on September 24, the angle calculated from the elements which have been given in this column differing from the observed angle − 4°. An observation on September 13, by M. Borrelly at Marseilles, presumed to apply to the satellite, must refer to a faint star, the satellite at the time being in the opposite quadrant.

BINARY STARS.—Dr. Doberck, of Markree Observatory, continues his investigations on the orbits of the revolving double stars. In No. 2,156 of the *Astronomische Nachrichten* he has given provisional elements of Σ 1768 and Σ 3121, the latter of which appears to be an object of special interest from the shortness of the period of revolution, which hardly exceeds that of the well-known binary, ζ Herculis. Also elements of Σ 3062, a star which was the subject of a pretty complete calculation by Dr. Schur in 1867. The results of the two discussions are as follow:—

	Schur 1867	Doberck 1877
Passage of peri-astre	1835·196	1834·88
Node	32° 10′	38° 35′
Angle between the lines of nodes and apsides	97° 31′	92° 7′
Inclination	29° 58′	32° 11′
Eccentricity	0·5009	0·4612
Semi-axis major	1″·310	1″·270
Period of revolution	112·64 years	104·415 years

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